Recd July 9th
Ans " 11

Germantown Pa
July 4th 1859

Dr George Engelmann
Dear Sir,

In writing to Dr. Asa Gray recently, I took occasion to send him a flowering specimen of what is known in collections as Cereus tuberosus, with the observation that it appeared to me to be an entirely new Genus. In a letter

Recd July 9th
Ans – 11

just received from him he says "I hope you will send the next flowering specimen you have to Dr George Engleman, he will I know be pleased and edified. From Dr E. remarks I think he would have considered it as constituting a new genus, if he had been sure the flowers were terminal, which from your specimen they appear to be."

I do not think I shall have another specimen this season, and so take the liberty of calling your attention to the plant as it is. My plant produced only four buds this season — two of which dropped before expanding. The two others perfected — one of them I sent to Dr Gray, and the other I left on in order to have an opportunity of studying the seed vessel

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which is now swelling. It differs from Cereus in having scarcely any tube, and that tube suddenly abbreviated, or contracted towards the base. In fact there is nothing in common with Cereus in the flower, but rather with Opuntia, with which it agrees in the property of opening only when the sun is fully shining on it, and of closing soon after midday, the same flower opening and closing many days in succession.

But the chief peculiarity lies in the stamens which are very short, and singularly *convolute*, forming a spiral twist around the pistil, which is terminated by a very bright green stellate stigma.

The flower is about two inches across, and of a cherry pink color.

As you know the ribs of the stem are clothed with short appressed spines, but the spines on the base of the tube are nearly a half inch in length.

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times in reference to any names
I may suppose inaccurate,—of
which I think I have many.

Very respectfully
Thomas Meehan

Did you ever notice that when the stamens
of a fully expanded Opuntia flower are
touched they immediately rise up towards
the stigma?

and very numerous.

I do not think I shall have another flower this season; but if you thought it desirable should be pleased to send you the first one I have next year.

My own collection of Cacti are now becoming daily more interesting, there being frequently rare ones blooming. If at all likely to be of any service to you I should be happy to send you flowers of any I may have bloom. Fifteen years ago I was very much interested in the study of Cacti especially, but the

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necessity of applying myself vigorously to business in order to afford me leisure for my favorite studies in future, have not given me the opportunities I would like - At the sale of Caleb Copes Collections, rather than see the Cacti dispersed, though I could ill afford it then, I bought the lot together. Since then I have been able to add to them somewhat, till now the Collection embraces, over 300 Species (Succulents of all kinds). If you will allow me, I should feel it a great priviledge to draw on your knowledge at

Rec^d July 24th
Ans^d Febr/9

Germantown Pa
July 12th 1859

D^r Geo Engelmann
Dear Sir,

Melocact?
Abgeschn.
Epiphyll. fr.
[illegible]

I am very much indebted to you for your kind reply to my former letter. My plant of C. tuberosus is swelling its seed vessel finely, and it will afford me much pleasure to forward it when mature — I do not desire the seeds for propagation, as I have reproduced a good quantity from cuttings, — I was only anxious to see it mature as a matter of information — If useful to you, pray

Rec. July 24th

preserve it. — My plant was originally given to me by a party, a friend of whom, I understood him to say brought it from Mexico; but I will try and ascertain positively. I am not posted up in the more modern discoveries in the Cactaceae for the reasons I gave in my last. When I originally studied them Pfeiffer's Cactearum was my text book, which with occasional stray papers in botanical works by Cels, Link, Otto and others, and my own observations were all the opportunities of information I had. I am much obliged by your references. I shall try to procure the works.

The ribs of C. tuberosus in my plant, can scarcely be said to run into the ovary, nor is the flower quite terminal. My plant showed buds as early as February, but the flowers were not fully expanded

before June - I do not
think they are regular in
this respect. I get Echinopsis
decora, tubiflora, ~~Eyyp~~ Eyriesii,
and some others to bloom after
a very heavy watering. My
Echinocactus tenuispinis
usually blooms in May. I
have a two year old plant
now showing buds, which
will I think be open in
August. - I send you a
flower and Mammae of
a Mammillaria I have
unnamed - marked in
my collection "Seedling"
and unlike any other one
I have.

It is between M. Odeiriana and M. r~~R~~hodocantha in my lot. It appears to be one of the most globose habit I have, although as my plant is but a two year old offset, it may be too soon to judge of its ultimate form.

I also send you a flower of Euphorbia antiquorum, which, though I have had under my eye for many years I have never seen blown before.

I have not yet Leuchtenbergia, nor do I

think it is cultivated in the
States. The only one
I think I ever saw
was in the Kew collection
at London, where (nine
years ago) they had several
specimens.

When I send the fruit
of C. tuberosus, I will
have a list of my
collection ready and
send with it.

And am dear Sir
Yrs much obliged
Thomas Meehan

I have.

MISSOURI BOTANICAL
PAPERS

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Rec. Febr 22
Ans March 30

[illegible]

Dr Geo. Engelmann

Dear Sir

It will give me much pleasure to send you any thing I can. [illegible]

I have been [illegible] the seed vessel [illegible] I am [illegible] easily from the [illegible] and with it I enclose.

[illegible] hardly [illegible]

[illegible]

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which show a shorter duration [illegible] [illegible].

The Epimediums have been in the [illegible] house with the Rosaceae [illegible] [illegible] and [illegible] [illegible] [illegible] [illegible]. I never saw an Epigynthum *trimaculatum* seed. [illegible] not many [illegible] [illegible] [illegible] [illegible] with the cultivators they have seeded freely. I have not ever seen the seed C. *parviflorum* [illegible] [illegible] & *C. [illegible]* [illegible] [illegible] C. [illegible] them.

Last year I visited [illegible] *[illegible]* [illegible] [illegible] [illegible] *[illegible]* [illegible] [illegible] [illegible] [illegible] themselves [illegible] [illegible] and the [illegible] [illegible] the

[illegible] [illegible] [illegible]
which I have yet seen.
All my Cacti [illegible]
[illegible] [illegible] with me
last year, than I have ever
observed them before.
Lost all my Echinocactus [illegible]
two winters ago by a little
water dripping through a
crevice - all the others [illegible]
[illegible] but they all rotted.
Wislizeni is no hardier than
other species. Before this they
bloomed and ripened seed
abundantly.
I have not yet got a
Leuchtenbergia. [illegible] [illegible]
[illegible] [illegible] [illegible].
[illegible] [illegible], and hope to
see it next season.
I have enclosed you a
[illegible] [illegible] [illegible] [illegible]-

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[illegible] lata. [illegible] [illegible]

[illegible] [illegible]. [illegible] [illegible]

[illegible] vessel and leaf in [illegible]

[illegible] — I send it because it

has always seemed to me to

have as strongly marked

generic characters distinct

from Aloe to which Lindley

reduced it as any plant

can have, — the flower more

[illegible] the fruit.

If you have any knowledge

of the Mesembry, author, you

may be able to name the

enclosed — my ideas are all

[illegible] blossom. Are any of these

of service to you?

I do not suppose the

[illegible] of Clematis [illegible] [illegible] [illegible]

to you, but enclose to fill the [illegible]

The synopsis [illegible] [illegible]

[illegible] [illegible] [illegible] [illegible]

[illegible] [illegible] [illegible] [illegible] [illegible]

[illegible] [illegible] [illegible]

[illegible] to you. I have a

passion for raising seedlings

[illegible]

[illegible]. I was the first to originate ~~[illegible]~~ the present race of garden Fuchsias having when a boy of 15 effected the first cross between F. fulgens and F. longiflora and raised them from a batch of seedlings ~~[illegible]~~ [illegible] Europe for three years afterwards. I have now up from each years seed, a batch of seedlings from ~~the~~ Echinopsis multiplex fertilized with E. Zuccarinii. I am fond of this subject more from the bearing it has on the unity of the idea of a genus than anything else and have had for years the idea

[illegible] has seemed to
[illegible] illustrate.
The last three years I set to work
freely to my knowledge of the
proper way of [illegible] cacti
& then leaving [illegible] and I am
the more inclined to watch
the changes in them since
I [illegible] take to [illegible]
years to arrive at the result.
My large plant of
Echinocactus [illegible]
occupying nearly [illegible] in the
pot, is covered with numerous
flower buds. I have never
seen this bloom under cultivation
yet, though I have seen dried
flowers from [illegible].
Very truly yours
Thos. Meehan

Rec^d June 4th   Ans Jun 12 —

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THOMAS MEEHAN, Editor.
GERMANTOWN, PHILADELPHIA.
—:o:—
D. R. KING, Publisher,
BOX 406 PHILADELPHIA P. O.

Germantown ~~Philadelphia~~ May 31 1860

Dr Geo Engelman
Dear Sir,

I am sorry that I have had no more Rhipsalis to send you. My arrangements for wintering my Cactaceae and Succulents, do not allow me to have many flowers at that season.

Now however they are beginning to show flowers freely. and I shall be pleased to send you flowers and fruit of any that may appear the following I notice to day showing bud,

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I open my letter to say
that I think I have
another flower bud forming
on C. tuberosus - if so
would you rather have
the flower. or try to have
a perfect fruit?
Echinocactus [illegible]
buds are now beginning
to swell.
Is the enclosed your
Myosotis inflexa

and if any of them
are desirable I may
say so, and will
forward some in
spring.

Mammillaria Wildiana
" redacta congesta
" discolor
Gasteria linguiformis
Echinopsis decora
E. mammillosus
Aloe verrucosa
Haworthia laevis

I will send
you lists of others
as they show bloom.

One of our
Philadelphians bids
fair to beat me in
his collection – Mr
Henry C. Gibson – He
got a taste for them

from seeing mine two years ago. He has already ransacked all the principal German and Parisian collections, and has half an idea of sending some one to Mexico to collect

Very Truly

Thos. Meehan

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Germantown Pa
May 9th 1865

Mr Henry Shaw
Dear Sir

I think I accidentally omitted to send you the Iberis sempervirens in the lot of trees I sent you. I send them by this days mail —

With them are a few things for Dr Engelmann.

Pinus pungens
Cupressus thuyoides
Pinus excelsa male flower
" but not open yet.
" Sylvestris Female flower.
Cryptomeria japonica old cones.

These are all I think I now have likely to be of any service to him

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as he is studying the Coniferae I may mention a curious fact which he may not have observed, — although of no possible moment beyond the fact that I know of — The ~~right~~ left hand line of the course of scales in a Pine cone goes all the way round the cone — making a complete circuit — The ~~left~~ right hand ones go only half round

owing to a slight difference in the lower angles of each scale ▽. The beauty of the fact lies in this that each angle line is so regular that we could not guess whether the growth which of course can only be in one direction, is from left to right or right to left, by the mere examination of the dry cones —

Truly yours
Thomas Meehan

Rec'd May 29th
Ans. Aug 17

Germantown Pa
May 23rd. 1865

Dr Engelmann

My Dear Sir

I am very much obliged by your kind explanation in regard to the twisting of Pine cones. I am very well versed in the main principles of Morphology, but did not know exactly how to explain the difference of angle between the right hand and left hand ~~Angles~~ lines. I understand it better by your note.

I have always given myself credit for fair powers of observation, with a tendency to generalize on the facts, and afford myself some foundations for laws of causes; but the want of an early education, and the means to procure

one when older, — having
been employed as a garden
laborer in England at
10/– or 2.50 per week, till I
was 20 years of age — gave
me no opportunity to systema
-tize my observations; or
know much of what other
observers had taken note
of. Since I arrived at
mans estate I engaged
keenly in business with the
main object of having the
means to indulge my tastes
should I grow older, and
though not yet quite "out
of the woods", I find an
annually increasing leisure
and enjoyment for observing
and for study.

I am afraid to offer many
notes of what I think
are original observations;

for fear I am occupying the time of my friends with what is already known to them and others, and it is only when I meet with an indulgent correspondent like yourself who is kind enough to set me right when wrong, that I venture to say much. It was only by D^r Gray's encourage-ment that I was induced to publish in the Proceedings of Phil^a Ac. of Nat. Sciences my paper on the relative differences between allied Species of Am. and European trees, and which I am informed very much interested Mr. Darwin and others interested in that branch of study.

This much by way of apology for what follows about

these pines were sent to you,
if the showed will interest you.

Fructification is of course
transformed leaves; but in
the Pine family it is arrested
fall growth which is formed
into male flowers, and arrested
spring growth into females

The leaves (bundles) of Pines are
but feeble shoots — the male
flowers shoots of a stronger growth
If the growing shoot is arrested
suddenly it produces instead
of leaves and flower buds, leaf
buds merely — I have pinched
partially back
~~off~~ young shoots when growing,
and the fascicles of leaves as
they would be under ordinary
circumstances become buds!
You will see this naturally
in the leading shoot of Pinus
Pungens sent. It was arrested
in its growth somewhere near the
summit*, and instead of leaves
on the afterwards prolonged part
they are concentrated as buds
at the base, — An earlier stage
of arrest, you will see on the same

shoot, produced female flowers — In the specimen of [illegible] Vine sent from the head the arrested to, are the female flowers — It is now starting to make another growth again this season, and when it ceases, by which time it will be fall, the arrested growth of that season will be male buds which will unfold with the first bursting buds of spring. You will see also in the Pinus section, the female is but an arrested branch —

In both cases — male and female flowers — there is an evident difference in the time and condition of growth necessary to produce the one or the other — I feel certain some general rule can be adopted made plain by

which deciduous plants
can be accounted for, but
beyond getting on this threshold
I feel I have not time or
opportunity to go farther.
In Carex the male flower
is the last stage of growth —
the maturing buds as it
were of the culm, although
its position on the culm is
the inverse of these on
the Pine. As in a
singular Lardizabalaceous
plant I had the opportunity
of examining this spring
— Akebia quinata — the
flowers of which are
arranged on a sort of
raceme, the first flower
that opens # has five
carpels and five sepals,
the next one but four of
each, and then comes the

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male flowers on the end -
In short is not a
male flower made up of
a more matured, and
less excitable condition of
growth than a female one?

I have only sent you one
female P. excelsa - I have but
few, and want to get seed;
but if of any use to you the
sacrifice will be of no great
moment, and you shall be
very welcome.

You will notice Pinus
excelsa male from one tree
has thicker cones and of a
lighter color than those from
the other tree. They are from
seeds direct from the Himalayas.
I call your attention to this
particularly as Dr Lindley
or Hooker has recently
announced his decision

that Pinus peuce of
Europe and P. excelsa
of Asia are identical
species, and you may
see some variation in even
here from the same place.

I send you one piece of
Pinus pinaster, which
I did not know I had in
bloom until I found it
half dry to day.

A friend in Baltimore
has Sequoia gigantea
fruiting; but they are all
now over — I did not hear in
time. If desirable he could
send you some next year.

I fear I have bored you
too much; but pray do not
occupy valuable time as yours
must be in replying should
there be little interest to you.

Sincerely yours
Thomas Meehan

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Rec Sept 8
Ans Sept 13

Germantown Pa,
Sep. 4th, 1866

Dr Geo Engelmann

My Dear Sir

I am much interested in the subjects of change of form in species, and have made observations which lead me to think that the seeds of change are latent in all plants, and have been there from the beginning, and that by a proper investigation we may some day arrive at such a knowledge of the "laws of change", as almost to see into the far future what the vegetable world will then be, — as we can now form almost an idea of what it looked

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* J. marginatus
J. acuminatus var debilis
J. scirpoides var Macrostemon

---

like in ages long gone by.

Dr. Asa Gray kindly intimated to me last winter that my views were worth publishing simply as a theory, but I prefer to collect facts for a while longer yet.

In connection with this subject ~~I would~~ it would very much interest me if you would be kind enough to name the specimens of Junci which I send you by to days mail. *

If you should happen to think them all one species which of the forms would you regard as the most typical?

I decided to send you the specimens after reading

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at page 426 of your Synopsis the Ac. St. Louis transactions, what you say about change of leaves in Juncus Scirpoides and J. ensifolius. My idea is that change being innate is not so much indebted to external circumstances as Darwin supposes; but not to bore you with my theories I may say that all the specimens I send you, are from the same circumstances of soil and moisture".

You say nothing of the "Roots" of Junci, when speaking of "characters". They have always interested me, and of as much value as the three stamens, which seemed to me but an abortion or suppression

which nature in her
apparently wayward mood, might
develop again at any time, —
or the terete leaves, which
seemed simply unexpanded
leaves of the same species, —
so tubers being but undeveloped
or suppressed underground
runners, or simple offsets on
peduncles, though ~~[illegible]~~ interesting
may be of no more value
in marking out sections.
I have seen Potatoes that
produced no tubers, but suckered
up like Raspberries, — while
Lycopus virginicus which
usually produces underground
runners, often suppresses
them into good sized tubers.
In one Juncus I send you
see well formed tubers, — in
another, simply thickened ~~suckers~~
offsetts, — in the third a slight
tendency to creep, with a

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Recd Octob 1st

Germantown Pa
Sept 26th 1866

Dr Geo Engelmann
My dear Sir

Not receiving the specimens I heard from Mr C. E. Smith that you had had an attack of Cholera; and scarcely expected an answer to my letter to you.

The Juncii grow altogether in a small marsh at ~~Cumber~~ Vineland Cumberland Co. N. J. The next time I go there I will bring living specimens and cultivate them that I may watch their development. I shall take much more interest in them

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than I have done since you decide them to be the species I supposed you would, — as I feel nearly sure they have all originally sprung from one type, and that by watching their development some clue to their mystery will be evolved —

What you note of the Rhizoma in Podophyllum is true of many ferns and can be particularly noted in the strawberry which I suppose you would class as an upright Rhizome.

The fibres come out only from the last years prolongation of the Rhizome, — all beyond that dies. —

Truly yours
Thos. Meehan

* I think I have seen these more tuber like, but cannot find them today

very little tendency to thicken tuber like.*

I have similar troubles with Juncus tenuis, but I am loathe to intrude on your valuable time to answer questions for me, which I ought perhaps to hunt up for myself. The greater part of my time has to be spent in the effort to make a living, and consequently the Bibliography of the Science is so neglected, that I hardly know where to look for the observations ~~infor-mation~~ I am often inclined to think new, but most probably already on record.

If you find time without much trouble to name my Junci, please refer to them as + the flat leaved, — the large one, — and the small. Truly yours Thos. Meehan

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Rec'd July 31
ans. Aug 5

Germantown Pa
July 26th 1867

Dr Geo. Engelmann
Dear Sir —

I am glad my note on the vitis interests you. I did not know the Polygamous character of V. vinifera had been so well observed. "Pretty well 'read'" botanists in this neighborhood still attempt to decide whether an improved grape is from a native or a foreign stock, by watching for Polygamous seedlings from it.

It is a doctrine with intelligent gardeners that

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flowers

unimpregnated, may though not always, produce edible fruit, sometimes with even seed which do not grow. I have been so taught, and ~~without~~ without going deeply into the matter have taken it for granted. I believe I have known Cucumbers raised early in frames, when occasionally a female flower will come before a male is produced, to produce a cucumber in palatable condition, — but at this moment I would not like to certify to it as a fact. Discussing this subject once with the late Colonel Carr, and Dr Wm Darlington, who both believed it possible, the

former said in substance, that
when the Osage orange first
flowered ~~fruited~~ at Bartram, the
trees were all female, but
they produced balls freely,
and had seeds, but they
could not get a single
seed to germinate, — after
some years some male
~~plants~~ shoots from some
other garden were grafted
on, after which the
seeds grew freely — and
I have myself observed that
female trees ~~from a~~
flowering a long distance from
male trees of Osage
orange produce balls
without seeds, except
here and there one in

some of the balls. The fleshy calices of the Osage orange are not to be sure quite analogous to the baccate ovaries of the grape, - but yet enough I presume to bear a little on the question. I shall however watch this matter closer hereafter.

I received the package of asters last fall with much pleasure - they are all doing well - I should like the Grapes - so much more can be learned from living than from dried specimens.

Cereus candicans flowered with me this ~~time~~ year the first time I ever saw it. The large globose fruit now ripening. If you should want one for examination, write.

Arr: July 21 1867

I am much interested in comparing Cactaceae with Portulaccaceae, as they seem to have a much nearer relationship than Lindley's vegetable kingdom affords them.

Going back to unfertilized seeds I am reminded that the researches of — to ascertain whether the seeds of Coelebogyne illicifolia had an embryo or not, seems to indicate that they supposed it quite possible for seeds to be produced without impregnation, or they would not look for evidence to account for it - although I believe that plant is now known to produce occasional pollen grains.

I have some idea of preparing a paper for the Proceedings to show that Myzà meldensis Lindl

is not a hybrid between the Red
Cedar and Chinese arbor vitae
as that author supposed, but
one of many polymorphous
forms the Thuja is of itself
capable of producing. During
my observations on the plant
I have noticed a curious circumstance: — The first growth in
the spring takes a curved course
due South, in a few weeks
it curves over itself by another
curve north, and then again
back south —

I have some reluctance in
publishing these papers. Passionately
fond of natural history when a boy
and young man, I had to abandon
the pleasure to battle with disease
and poverty, and during the last
three or four years that I have been
able to take up the broken thread, I
feel that I am so liable to write
as new, what is most likely already
known: It is only that I feel our
"proceedings" have so little of botanical
interest in them that I venture
to offer any thing at all..

Truly yours
Thos. Meehan

I enclose shoot of Thuja meldensis

Rec^d July 31 1867

P. S.

I thought you would like some Vitis Labrusca fresh instead of dried. I send some to day. They are from two plants growing within a few feet of each other in a waste fence row. ~~The~~ One had a large quantity of racemes without a berry as you will see. The other have some berries but many imperfect ones – I think the fox grape never "sets" all its blossoms.

I think you will also observe ~~the time of~~ the leaves have light blotches through them, which perhaps if you place under a strong microscope may prove a parasite in process of development

Recd Aug 26

Germantown Pa
August 22 1867

Dr Geo Engelmann
My dear Sir

Anxiety for my children through severe Scarlet fever kept me in the house until the pod of the Cereus candicans burst and dried. but I will bring the remains to St Louis, where I hope to be during the Pomological convention. I leave on Monday next for the west. The Cereus is a nocturnal flowerer, the Petals are short in comparison with the length of the tube, with the stamens as long as the petals. The fruit is depressed globose, turning (here) of an orange-brown color.

I enclose you some seeds of Picea balsamea gathered

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Missouri Botanical Garden

to day from two different trees. The purple seeded one is the form I have always seen and considered normal, — the white seeded form I have never seen before — on removing external cuticle to the seed proper you will see that ~~the~~ it is quite unlike the usual fir seed, and approaches very much the form of the Norway spruce — I send you a few Norway Spruce for comparison — There are many ~~hundreds~~ scores of Balsam Firs, and Norway Spruces on these grounds, some of which are entirely male and always produce nothing but male aments, — some only produce a few female, — some many female, — but all I believe

have more or less males, whether
they have females or not.
This year only two trees
~~produced~~ matured any female
cones (~~[illegible]~~, I do not know
whether they produced female
ones or not in the spring)
Is it possible that the tree
bearing these pale seeds
has been fertilized by Norway
Spruce Pollen — a late
frost or some other cause
destroying its own?

I have never known the
Balm of Gilead mature cones
without fertilization — that is
I have not observed whether
they do or not, but the
Norway Spruce does, which
I had forgotten till now, — another
instance to add to yours of the
Pine apple. These seeds are
never good in these cones.

The observations on Thuja Meldensis are on 25 plants growing side by side on my grounds: all have the same peculiarity. Since the first of ~~August~~ the curves have commenced straightening, from the bottom upwards, and the "kinks" have now nearly disappeared except the tops which to day are all curved towards the north except two plants, which have straightened out entirely, and are now taking a perfectly upright — I think I can account for the ~~dich~~ distichous growth of the arbor vitae, by this peculiar behaviour of the plant, but am waiting for further observations.

Very truly yours
Thomas Meehan

Th. Meehan

Rec Dec 30th
Ans Jan 7

Germantown Pa
Dec 24th 1869

Dr. Geo Engelmann

My dear Sir

I was last week in Ottawa Illinois, and I gathered there two very distinct varieties of Opuntia vulgaris, which I now send you. The round sections I found on the "Buffalo Rocks," about one hundred feet above the Ills. River, — the ovate form and long fruit, is from the farm of Mr. Thos. Barnett, the mathematician, who complains that the spines make it uncomfortable resting for his cows in summer; — his pastures are full of it. The round one is very nearly like the ones I got three years ago on Piasa Bluff above your town, and which you saw in my garden. The other is like the common one you saw also here, only for its spines — enormous spines, and very long fruit. I have many things I would like to say to you but am sure you must have enough to do to catch up with many things which have awaited your long absence for attention.

Very truly yours
over
Thos. Meehan

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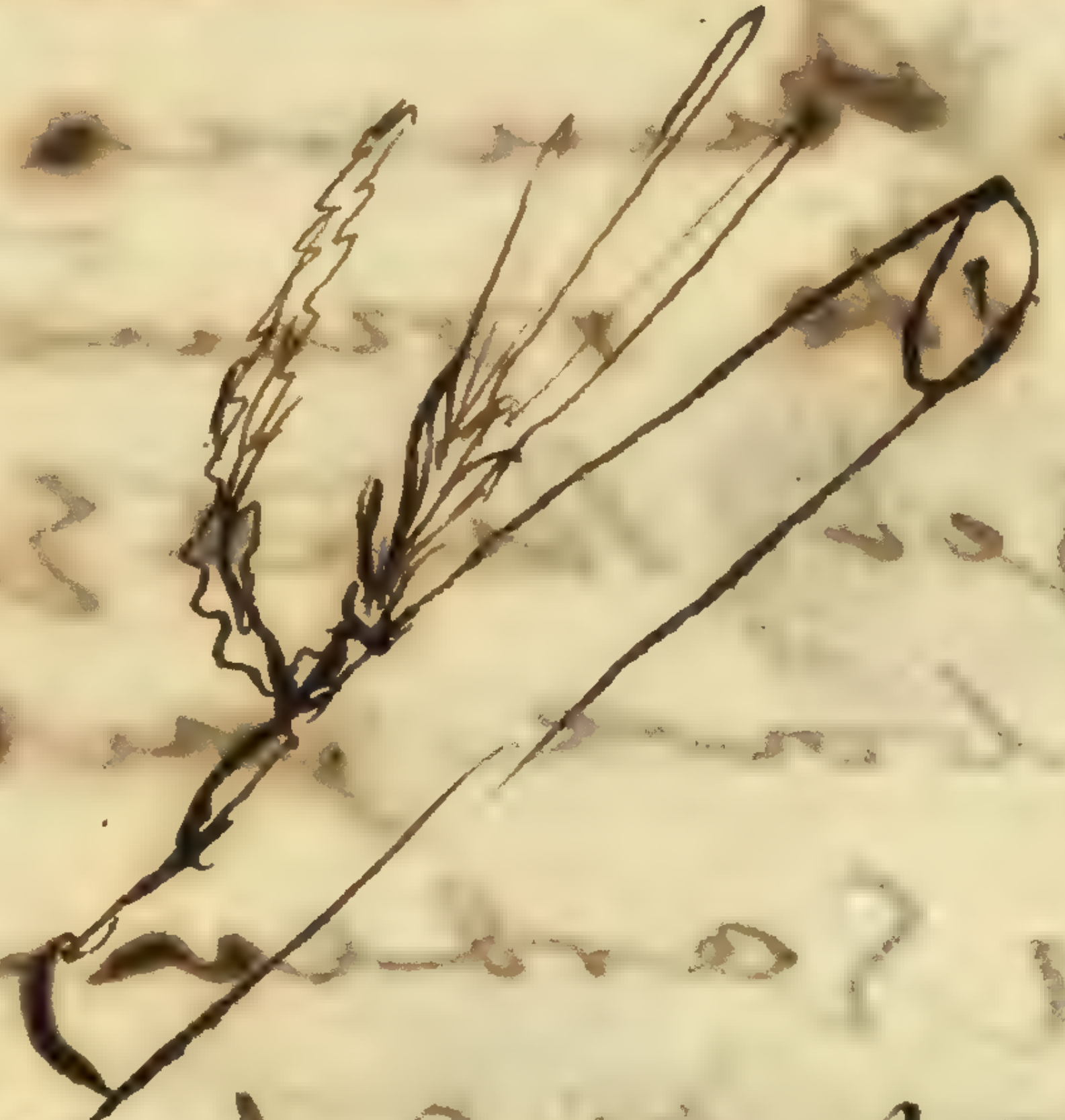
I cannot help adding that I have a very interesting Pine specimen sent me by Mr Ravenel who found it after reading my paper on coniferæ leaves.

The scales have developed into "foliaceous cones", and these are serrate as you described the leaves of the Norway Spruce to be—



This gives the idea very roughly — I shall have a few remarks on it for the Academy next Tuesday, and ask them to figure it in the Proceedings.

Mr Lapham of Milwaukie was with me and promised to send the Opuntias, but for fear he forget, I send.

Recd Jan 18th
Ans Jan 19

MISSOURI BOTA
GEORGE ENGELMA PAPERS

Germantown Pa
Jany 14th 1870

My dear Sir.

I notice that Decandolle has quite recently endorsed Browns theory of the structure of coniferous flower. It cannot be correct if my theory of Pine leaves is the true one. Has it ever been demonstrated by actual microscopical observation that the ovule is produced from the upper surface of the scale in Pinus? So far as I can understand by cutting open a large number of cones, they proceed from the cellular matter underneath the scales, and the mark on the scale beneath, on which the seeds appear to lie, is but the result of pressure. This cellular matter (bractea) is the analogue of what I call true leaves and although I doubt whether the leaf has any developing influence on a bud, yet it is usual for them to appear in connection. The scale being but a metamorphosed branch and not a leaf, it is unlikely that a

Seed would be found above and in their axils. My observations on split cones, renders this almost certain, but I cannot trace the matter up so close as to be positive. I thought you might know whether these ovules had ever been noticed by the microscope in any certain position.

Again is it certain that the the female flowers of our american Pines are fertilized the first year.? By noting the direction of the woody fibre in these cones, there seems to have been not only a remarkable cohesion of parts in infancy, but a curious doubling up or inversion, as I pointed out to you in the "stipules" of magnolia, and if this is so, it will account perhaps for the Anatropous ovules. In cutting open some Cones, I notice in some species ~~[illegible]~~ there is a large oil cell at the base of the scale, in some it is not clear, in others plainer than those vesicles. In some not seen at all; but always when present in the same place. It is curious that on the back of the true leaves of Juniperus often is ~~the~~ a similar "gland"; — and that the seeds of Pinus come out (as I suspect) from about the same position on the bractea. Can there be any relationship between these oil cells and the ovary? (for I half think there is an ovary in spite of Brown) I suppose you have seen the oil cells on the seed of Picea. It is difficult to clean the turpentine out to see them nicely, — but of the few I happen to have bye me I send you one, — for I never met any one who has seen them but me, but you have studied them so much closer than I have. It always seemed to me I could divide ~~the~~ Picea from Abies by the seeds alone, — not

only from these oil cells,
but from the fact of the seed
being surrounded by a
Carpellary scale, while in
abies they have the scale only
on one surface —

Very truly yours
Thomas Meehan

I learn Dr Gray came back
last week ——

about Pinus pungens - usually
it has two "leaves", — But we
were speaking about the ~~waves~~
Waves of growth, each weaker
than the first of the season. I find
that the last wave produces
three leaved fascicles. Has axial
vigor aught to do with number?

I have tried so often to tear off a scale
so that the seed attachment showed not pulled off,
and have now for the first time succeeded,
I send it to you, but in trying it came off. The
union is by the two legs of the "Carpellary"
scale to the two little protuberances of the
transformed leaf.

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Rec^d March 1st
Ans Octob 15th

Germantown Pa Feby 21/70

Prof. Engelmann

My dear Sir

I was verry much interested in your suggestions on ~~Pinus~~ the inflorescence of Pines. It seems to me that some facts that I have observed are still inconsistent with the explanations; but I want to examine still further this season.

What I want to send you now is a singular fact in the duplication of buds, in which as you know I have been interested ~~in~~ so long. While with my friend Josiah Hoopes yesterday, I noticed on some very vigorous young *Magnolia acuminata* plants, that there was a lateral bud uniformly on the right side of the one in the axil.

Examining my plants at home, I could see no lateral bud, but on examining the wood I found that there is one under the bark. As fast as the tree grows the ~~bud~~ lateral one grows so as to keep always under the cuticle (pretty much as I pointed out to you the buds of Gymnocladus do). I at once examined many other trees to see if others had similar buds, and I find on ~~many~~ ten or twelve year wood of Celtis, that there are a pair of laterals, distinct from the central bud, going along together and only becoming apparent as the branch increases in age. I thought you would like to see specimens. I suppose you will see as I do, that these facts do not help the usual distinctions between axillary and adventitious buds.

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Recd March 7th

2

Moreover I have never been satisfied with Nuttall's Gray's or Lindley's explanations of the formation of Bark. I never could see why it was not as necessary for a Beech to "crack its cuticle" with growth, as an oak or Elm. Nor why "cracking of the cuticle" should produce those broad scales in in *Aesculus hippocastanum*, and pure and beautiful cork in the allied *Aesculus glabra*, and so on — and I have strongly suspected that all this bark exfoliation is the result of as peculiarly a specific law of vitality, as the production of leaves or stems. But I can find nothing in any book, which hints at this. Can you direct me to any treatises on Bark formation that would interest me?

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The true ~~theory~~ plan of exfoliation is very clearly set forth in Sassafras. Diamond shaped cork cells are formed on the young shoots. On two year old, these elongate, and cork fibres are developed, under the outer pellicle, which is destroyed in their development. The elongations touch the third year, while the cork fibres thicken, or rather seem to addl density by numbers, and by the fourth year long rows of corky matter appear, which make a new layer, underneath the other every year. Sometimes this corky development kills the bark, almost in to the wood, but when these cork cells are not developed the bark lives as long as the tree lasts — not a particle dies ~~one~~ each year or any year. A layer of liber is made every year, which

Recd March 1st

never changes its nature to become wood as some think, but it can be counted in the bark of oak and chestnut and other trees, after it has lain one year, and the exact number of layers tally exactly with the number of concentric rings of wood, except when destroyed by the development of cork cells. Cork cells develop usually when the tree is from 15 to 25 years old, although as in the sassafras and some other trees they push on at once.

This seems to me to be true, and I have wondered for years how it can be true if a new layer of bark is formed next the wood? A Chestnut for instance certainly never loses a particle of its bark until it is twenty years

Rec.d March 1st

old. Then it will be about ~~twenty [illegible]~~ thirty six inches in circumference. There ought to be a crack in the bark somewhere before this if the ~~layers [illegible] in the~~ accepted theories are true. It seems to me to prevent this cracking there should be a lateral addition every year to each annual layer, as well as the production over the other of a new one — But you will say logic has nothing to do with physiology — we must have the "facts", and these I have not got, only — I am not satisfied with ~~the~~ my early teaching.

Trusting you will have patience with me I am very truly yours

Thos. Meehan

Paulownia has double bud, one above another - the upper the largest -
So has Nyssa sylvatica.

Recd April 9th
Ans. Octob 15

Teratol — 187?

Germantown Pa
April 4th 1870

Dr. Engelmann

my dear Sir

When you were here I gave you a specimen of a twin osage orange. We have found many of them this season. In one case ~~however either~~ one of the twins has either died ~~in~~ (its ascending axis) or it has become thoroughly incorporated with the other stem.

I thought you would like to have it as a companion to the one you have.

With kind regards
Truly yours
Thos. Meehan

Rec'd July 13
Ans'd Octob 15

Germantown Phil. July 8th 1870

Dear Dr. Engelmann

You know when you were here you opened my eyes to an exception to what seemed to be universal rule, namely that the lower bud in the longitudinal series was the largest in *Xylosteum*. I have since seen that this is so in all *Loniceras* and *Caprifoliums* I have examined, — also I have since found that these longitudinal series are to be found in *almost all* ~~plan~~ woody plants at times but so far I have found none like these Caprifoliaceous plants. It has puzzled me no little, and I have been watching them continually, to try to account for it.

I have now found that in the *principle* of their formation they are probably no exception. ~~I~~ The upper or large bud of Itea produces the flower when the two buds are present. So also do the

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for want of the direct evidence, and I have to wait; but I see by "Nature" that some other one, has proved what I suggested to you, that Pinus could not be a Gymnosperm.

So also I note that Dr. Spruce and others are working to take the credit of the theory of sex, which I advanced last year — namely that it is not the highest efforts of vitality which produces the male sex as Prof. Murray supposed, but the reverse. Spruce says he came to the conclusion "25 years ago", and "intends soon to publish a paper" to prove it. Isn't his caveat too late?

Truly Yours
Thos. Meehan

upper large buds of Halesia tetraptera - in Lonicera it is still the large buds which flower, though the position is reversed.

I think I shall find out some interesting laws in reference to these matters someday, and keep on making observations with a great deal of interest.

I satisfied myself that you were right about the early impregnation of Pinus, but could not get about the morpho-logy. But the parts are so small, and I can only argue that if I am right in my views of the morphology by simple adnation, prevailing theories of cone structure must be wrong; therefore I can make no progress in the certainty of my points

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THOMAS MEEHAN, Proprietor,

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Rec. Oct 29
ans March 16 1871

Germantown Phila Oct. 26 1870

Dear Dr. Engelmann

What a *beautiful* cactus! I shall do my best to raise it. So with the acorns; but are you sure about the hybridization? In my own experience I have lost faith in them, and believe they rarely occur in nature *if at all*. It seems the normal condition of our atlantic oaks to have lobed leaves; and the entire leaved ones seem occasionally to resume, or attempt to resume, a character they have lost. Dr. Gray gives me credit in the Manual for discovering a plant of Q. heterophylla in Delaware; but I never considered it more than an accidental lobing — just as the paper mulberry will do when its powers are not strong enough to develop all its veins on which the more entire leaf depend.

After you were here and my Opuntias ripened their fruit, I found that the seeds of the two were identical; and remembering what you said on this subject I was puzzled. I sent a man some years ago to New Jersey to gather tree seeds, and he brought this Opuntia back. I *understood* it was wild. possibly from an old garden. I will try and find an undoubted New Jersey locale.

This longitudinal bud subject is proving more interesting every day. I now find to a certainty that the appearance of the peduncle in many Solanaceae between the nodes, is a case of this character

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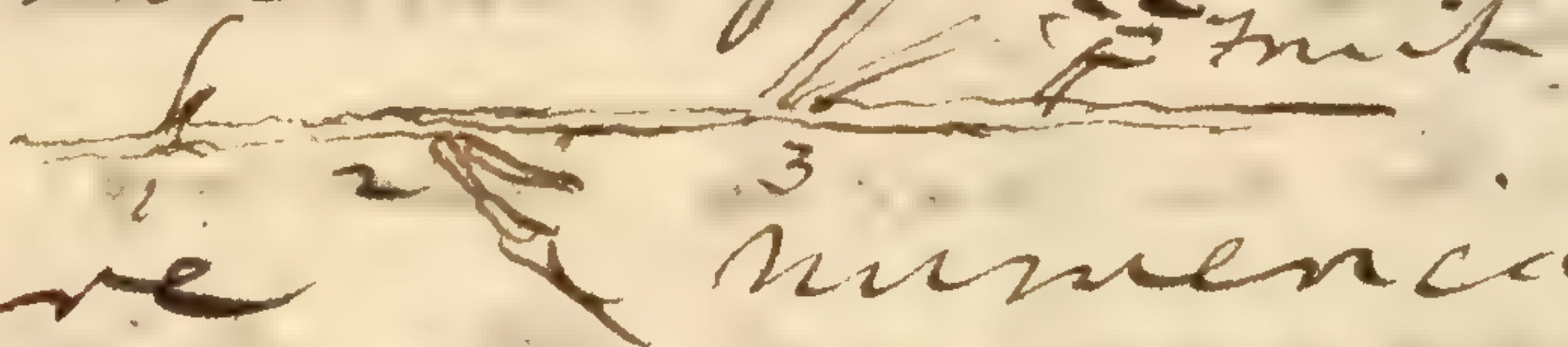
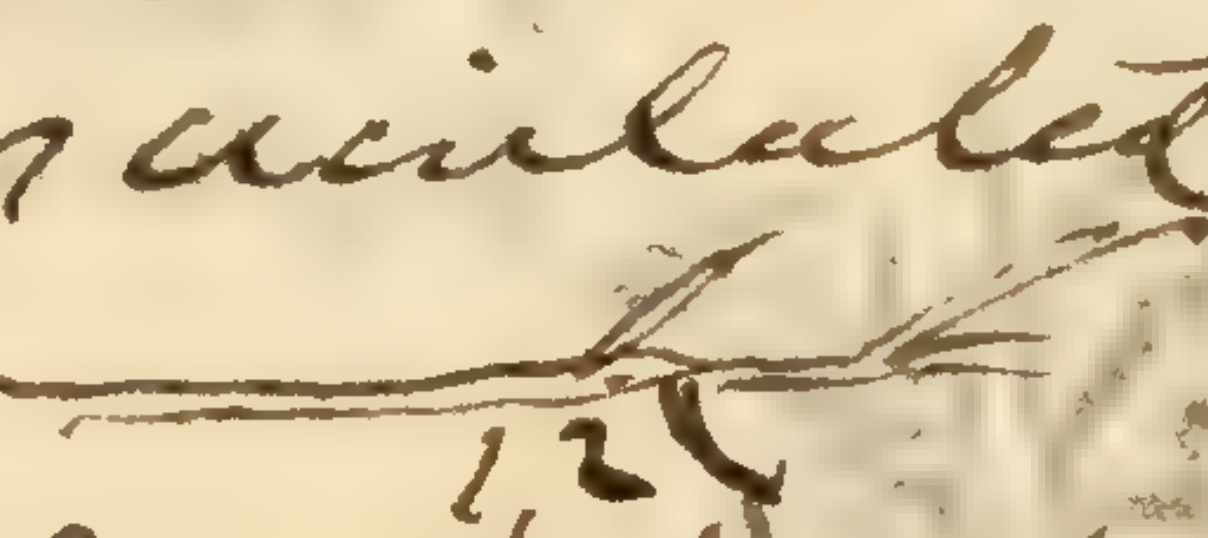
the peduncle is merely from an upper bud in a series of two which belongs to the node below. In the Cherry Tomato it is best seen, as it sometimes is found almost down to the lower node, and even when nearly up to the upper, if torn back, the fibres can be traced back to this node. This observation led me to note that the peduncle in all Solanaceae that I have been able to examine proceeds from every third node, and 

that the axillary buds are numerically weaker: thus 1 leaf has scarcely a bud visible; 2 stronger axillary bud; 3 very strong pushing into a secondary shoot in fact, and above this the "extra" or "super axillary" producing the fruit. In some Solanae the nodes get fasciculated; in Nycterium veslaceum 

1 and 2 being close together. In Datura suaveolens all three together, and the peduncle bursts out between them although evidently the fourth production of the dense whorl. Now the interest of all this is that the same phenomena exists in Phytolacca except that I have not found the peduncle so variable in position. It extends sometimes a 1/4 inch above the node opposite, but I have never found it below this upper node as in Solana. Is there really much difference, ~~between~~ after leaving out the adnation of stamens with the corolla, between Phytolaccaceae and Solanaceae, except such as may arise

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3/ from abortion or multiplication of parts? I made the suggestion last summer to Dr Gray; but he replied "how absurd! It shows you know nothing of the principles of botanical Classification". Whereat I was for a time frightened. But I keep hacking away at every specimen I get, and somehow in spite of my desire to accord with so good an authority, I cant get the idea out of my head. Today for instance I cut across a half ripe berry of Solanum capsicastrum, and find it ten celled; and the peduncle of Datura suaveolens has 4 bundles of fibrous tissue in tens so that Dr Grays Characters in Manual, "fruit 2-3 rarely 5 celled" is not absolute. So also 5 androus flowers. The Tomato often has 10-15 - and once I found 25 - although still in the 5 sets.

Following out the hint you gave me last year about grape vine tendrils, I find ampelopsis almost always has its third node without a tendril; and ~~those~~ the two with tendrils without axillary buds. There seems something of the case before noted in Solanum. Query Can the tendril of a grape vine belong to the node below? It seems scarcely possible; yet how can it in phytolacca find itself directly

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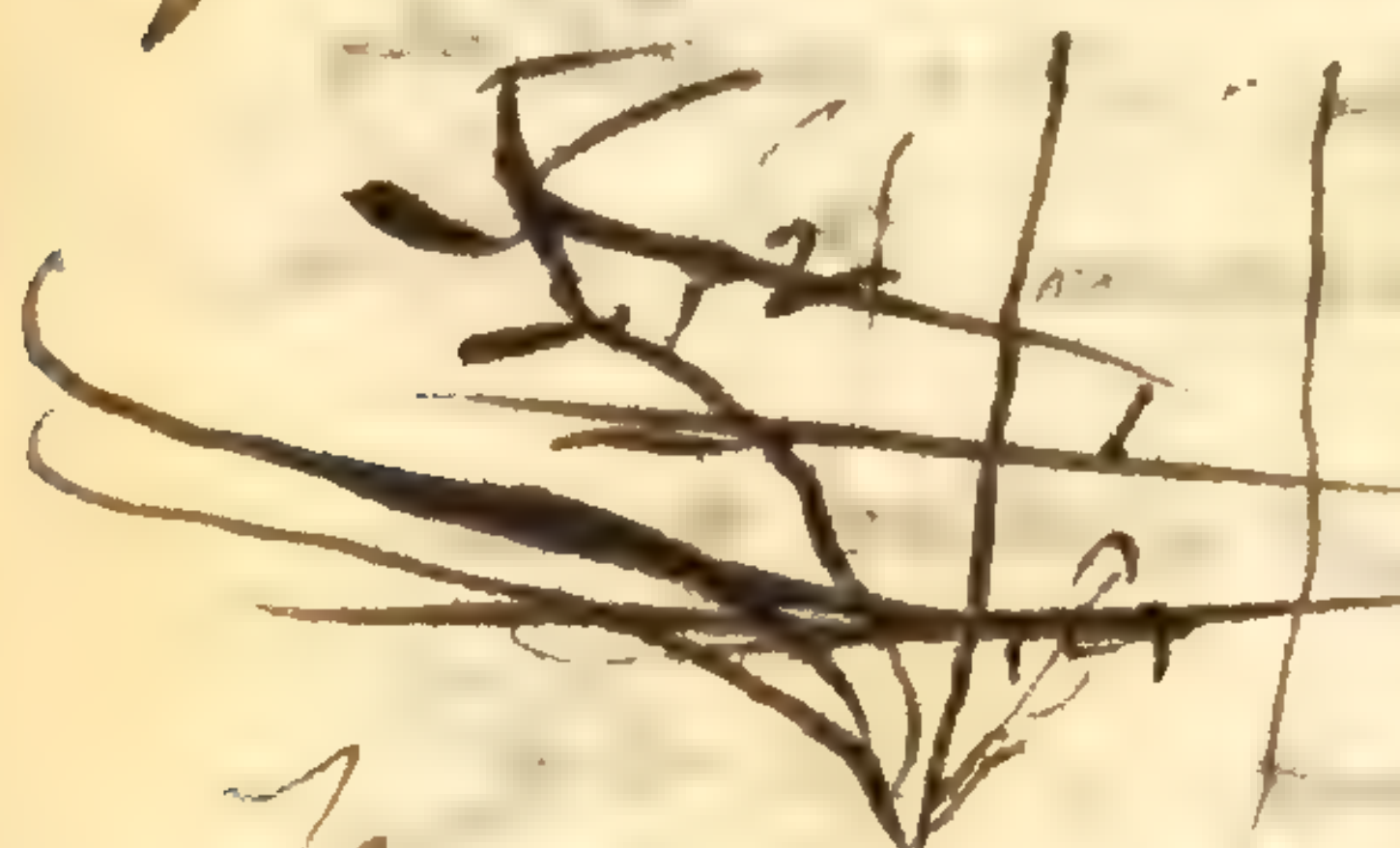
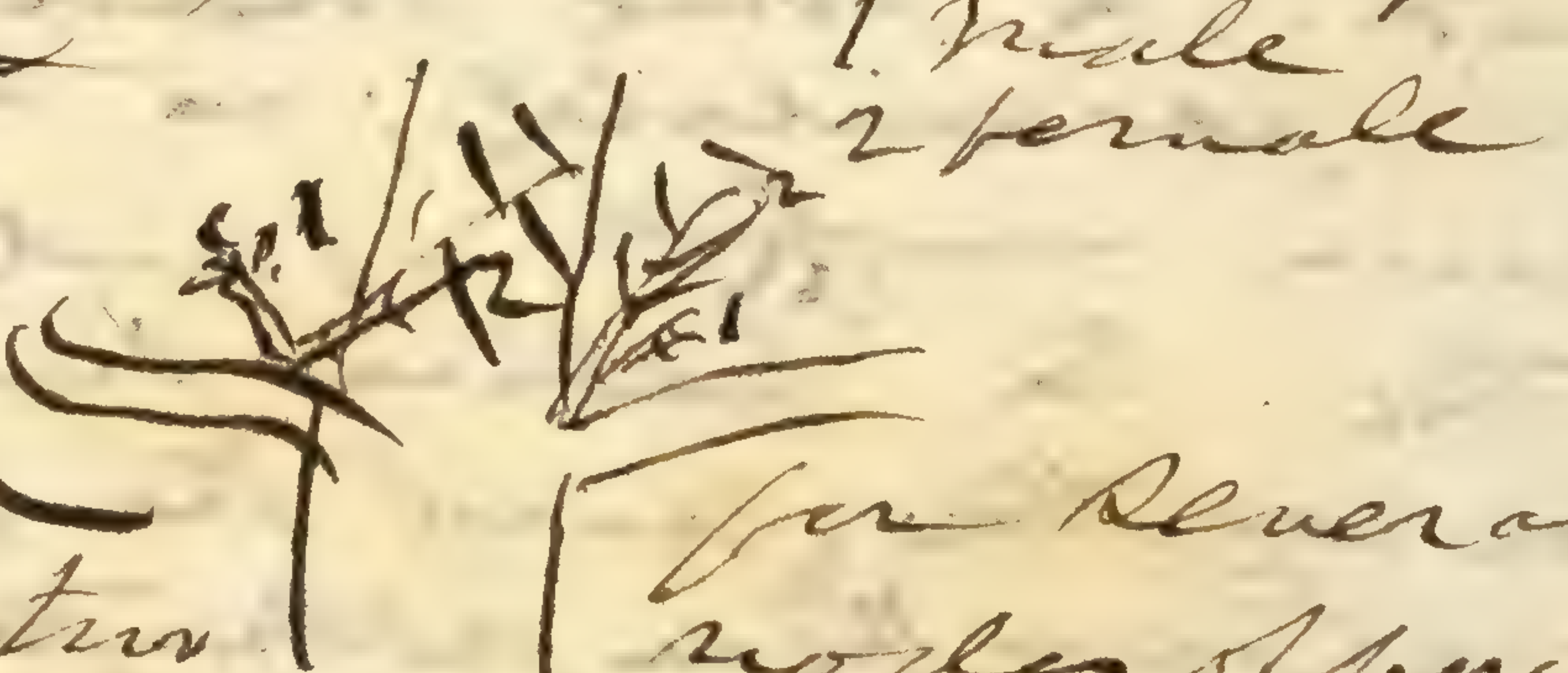
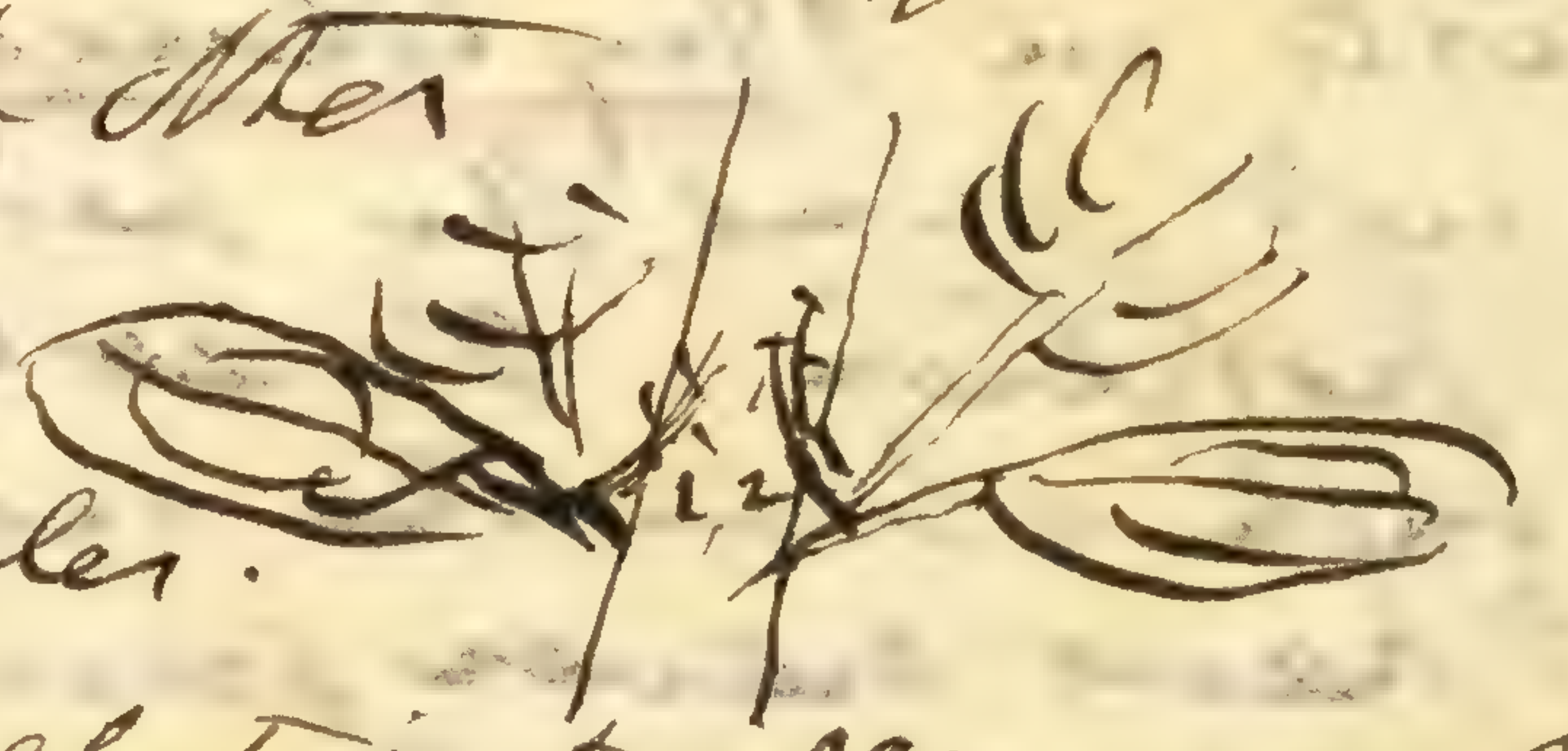
4/ above the lower axil, as in the Solaneae? It does not seem right, and yet I see no variation from exactly opposite the leaf, and at the node, as I do in young tobacco and Solanum.

You suggest a change of position sometimes in the odd bud of Magnolia — I found it was so this year in Boehmeria cylindrica. You know that the weak pedicel which bears the male flower comes out at the side of the ~~str~~ strong one which bears the female. This is usually on the left, or upper side (If I remember right) of the spiral growth.





This continues on for several nodes, when the two nodes opposite bear the small male pedicel one at the left the other on the right, so that they face each other



1. 2 males.

In relation to sex, and the term vitality, I have often thought of your objection indeed it has often been made. But I have no other word that comes near to what I mean. I mean by vitality simply life and I mean by a high grade of vitality a condition far removed from death, which in my Vitometer I would call zero. — When plants are growing vigorously, I do not take it that they possess so much vitality as when they pass this stage and commence to bear, — even if the reverse of this were true, it would make no difference to my theory of sex for that deals only with the conditions of vitality at the *reproductive age*. If the growth as you say should always continue "so strong" as never to bear fruit, simply, then my theory would be inoperative. It would not be a question of whether the plant should bear a male or female; but whether it should bear at all. But when it did commence to bear, I take it that whether it produced male or female flowers abundantly would depend on whether the sources of nutrition were turned into the channels of those buds which produced flowers, — or the degree of nutrition which flowed into the buds. If full nutrition went to a certain bud it would be female, — if impaired nutrition male.

We always raise Cacti by decapitation when they do not sucker.

You say the "Law of expansion" in bark — there is not the slightest degree

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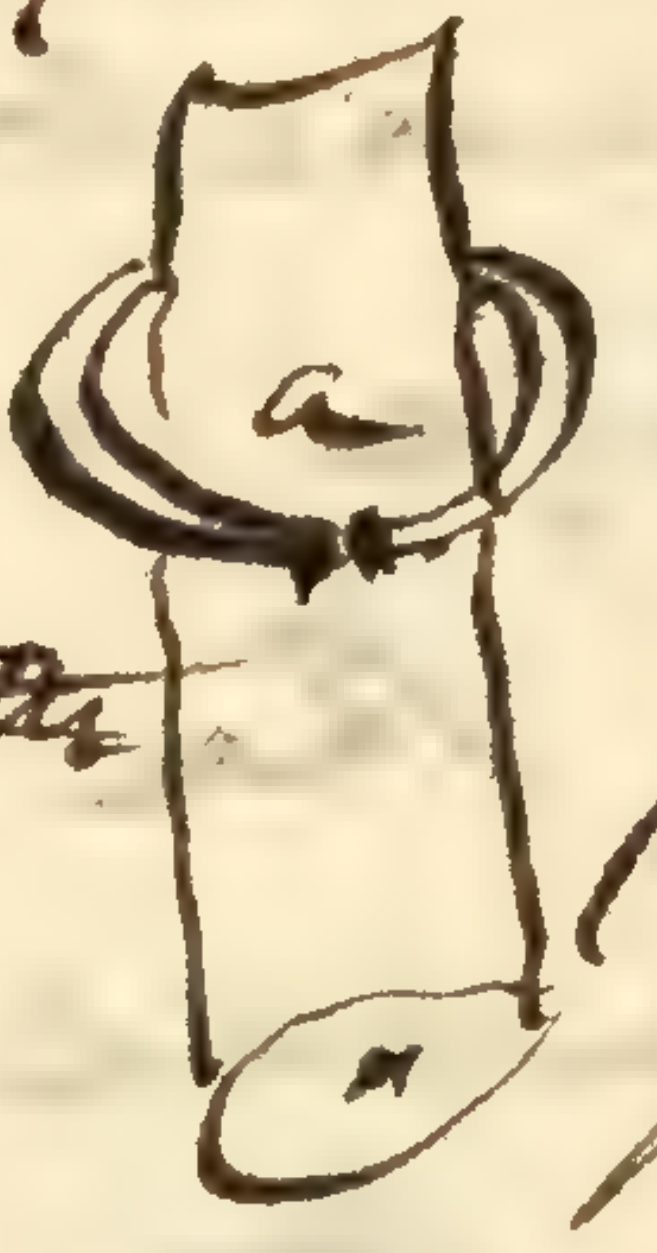
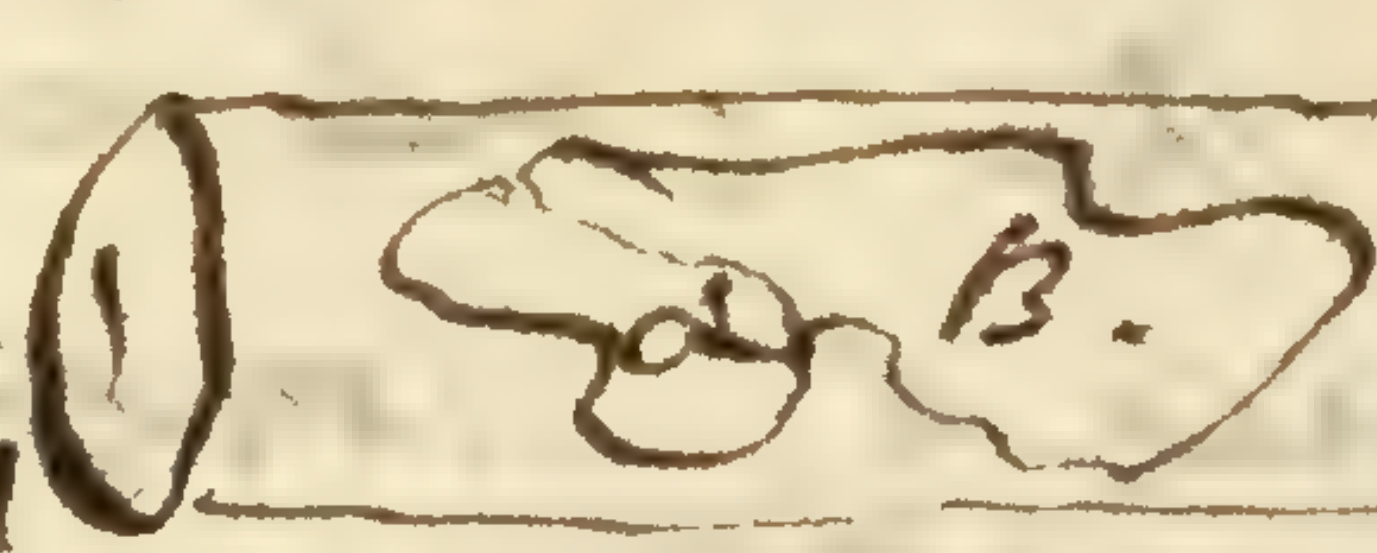
of expansion. This I have tested.
If you put a piece of tin or wire
closely around a tree so that
the edges meet at - as the tree
grows it will



not open at
a, but will ~~grow~~ grow over the bark the
same as if the ring was
united continuously — also if
the bark — the external cortical
layer (as well as the liber — in the
common chestnut does not die till
the twenty fifth year — In the Beech
it never dies. why has not its expansion
split this bark before this time? In
the Platanus, Linnice, and some
others, a patch of bark (a) will
peel off this year, another patch (B)
peel off ~~this~~ next year, yet all
be of the same age



My idea is that bark dies
by the development of cork cells — perhaps a sort of pseudo fungi — not by
expansion, or necessarily by age, — and
I was anxious to know whether any such
an idea had existed, — or anything of
~~such a nature~~ consequence in the true
nature of cork cells had been published

7

If nothing of this has been published I thought to put to letter a series of observations on various barks, with the periods of their individual con-[illegible], and some other facts, for the American association next year.

I am a little hurried just now with fall business; but thought best to write at once before the thoughts suggested by your kind letter passed from my mind.

Yours sincerely

Thos. Meehan

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Rec Nov 28 1870
ans March 16 1871

Germantown Phila. Nov. 25/70

My dear Dr. Engelmann

A severe tussle with diptheria in my family, in which I lost one of my daughters, and others scarcely yet out of danger, has led me to forget until to day that three weeks ago through the kindness of our mutual friend Mr. Burk, I got a piece of the Opuntia from its native locality Woodbury New Jersey, where I supposed, but did not like to say positively mine originally came from — Mr. B. says there is at least an acre there, and he is sure no escape from any garden. Still I should suppose it must have been introduced there some time within the white man's advent, or a plant which is so well able to maintain itself against most ~~all~~ enemies, ought to occupy more territory in thousands of years. Still as Jersey has been mostly one vast Pine forests, and I suppose the Opuntia would not live in such dense shade, it *may have been* confined to a limited spot by the *shade* enemy. Certainly as you say, the characters agree with your

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figs and descriptions of O. Rafinesquii in the Boundary report, ~~and then~~ with O. vulgaris, and I am very much interested in knowing what the real O. vulgaris is. Is it not more than likely that these plants vary very much from seed without any hybridization, — as we find it in other allied plants (the gooseberry for instance) and that they are all forms of one thing. I think these plants very rarely increase from seed in their own places of maturing them. The young plants probably being crowded out by the old ones. In my own Rockery where there has been every chance by the annual ripening and decaying of the fruit, I have never noticed a self sown seedling plant. On the other hand if a seed or fruit get carried away some distance by bird animal, or any external agency, and get to grow, varying a little from the original a new centre of another variety is formed -

Recd Nov 12 1870

Thus on the farm of Mr. Bassnet at Ottawa Illinois the large patch had all somewhat ovate fronds,— while a mile or so away, on the Buffalo bluffs overhanging the Illinois river, they had very round fronds. So on Piasa bluffs above Alton (I saw only a few scattered plants here to be sure). The few plants I saw had no trailing tendency, but the fronds were set one above another in an erect position, and the number of small white spines was most striking. Yet the little piece I brought home is now a bush of a foot or more over, and has no ~~[illegible]~~ erect tendency, though retaining its spininess. In this case probably the erect habit was through growing amongst underbrush. The two little pieces brought from the Illinois River and Ottawa, have also branched the last year,— so that I can send you fronds from all these three locations if you wish. Although now three summers since I had the Piasa one, it has shown no disposition to flower yet.

I have a pretty mammillaria from Colorado. It was only about 1/4 inch high when I got it last year. It is now about 3 inches by one broad, perfectly columnar— Skin dark green— mammae tapering - the lower series of numerous bristles white - the stronger (5 to 8) aculei somewhat rufous - Probably it will flower next year.

Very sincerely yours
Thomas Meehan

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ans Aug 16

Germantown, June 20.

Dr Engelman

found Aug 15

my dear Sir

Two young m
of our Academy, with no
but with some mean
have decided to devote
themselves to a great w
of which the enclosed speak.
The one is already known fa
vorably as a successful li
grapher and artist, with a
very large business, – the
other you know for what h
has done in Conchology.
is also a fair botanist with
growing love for it. They a
young enough perhaps to see
the end of it; but I hardly
expect to; but I have deci
to aid them all I can wi
out any hope of reward.

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Germantown,

If you feel justified in
a word or two of encou
ment in such an en
prize, they will high
appreciate it.

Very truly yours
Thomas Meeh

S,
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Horse Cars along Eighth Street
direct to the Nursery.

THOMAS MEEHAN,
Proprietor,
Office, 5547 Germantown Avenue,
PHILADELPHIA.

Rec^d June 27
Ans July 4

Germantown, June 24 1871

My Dear Dr. Engelmann

You say you are a bad correspondent, but how is it with me! But I have made up my mind for a run of a few weeks in the Rocky mountains, jumping off the rail at Denver, and I naturally think of you before I go (July 15th).

1st Buckley's paper — I fear there was some misunderstanding with the printer as to "extras" and I do not think Buckley got any. I do not think his paper very strong, and only a sense of ~~permitting~~ the justice of affording him the chance of putting himself right if he could, induced me, as one of the committee to whom the paper was referred, ~~induced~~ to pass it. I have told Mr. Buckley his best defence would be new and better specimens, and he has promised to try to get "Hoopesia" this year. Yesterday a letter from him said he had engaged a friend at Port Lavaca to go to Corpus Cristi for it, but I fear the distance is to great for one who is no botanist — only a plant lover to go.

The seed of the "hybrid" oak you sent me, I have raised, and I shall enjoy watching it.

In regard to lobed forms of entire oaks, I feel nearly sure, that if you will cut a ~~plant~~ a plant of several years old, down to the ground you will find lobed leaves on the next seasons shoots. I have had no experience with this species, but I have on so many other things, that I hardly think an exception will be found here! I understand what you

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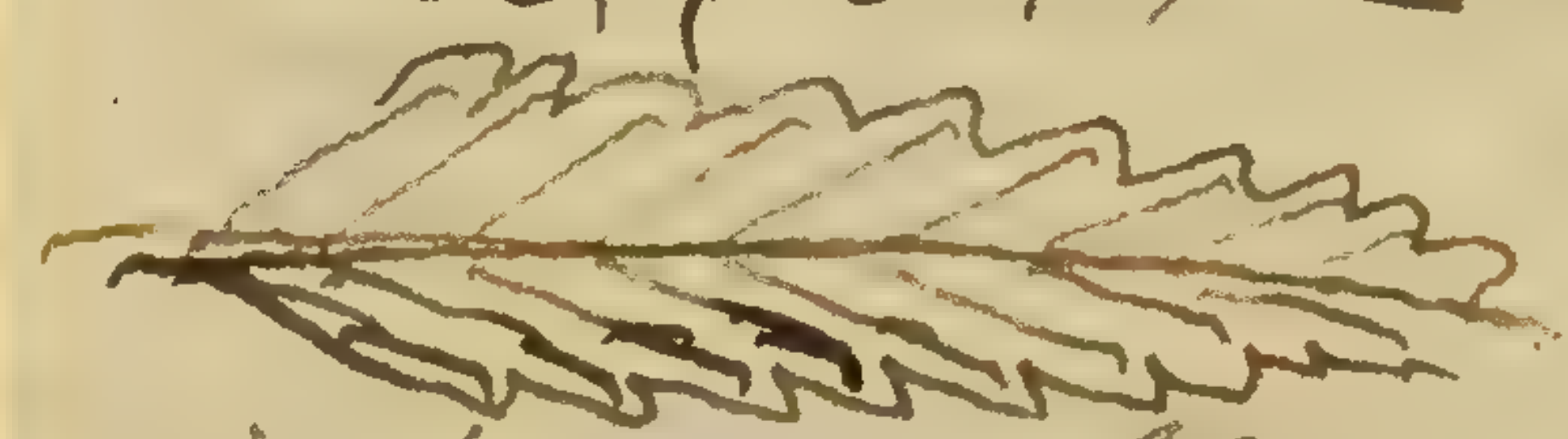
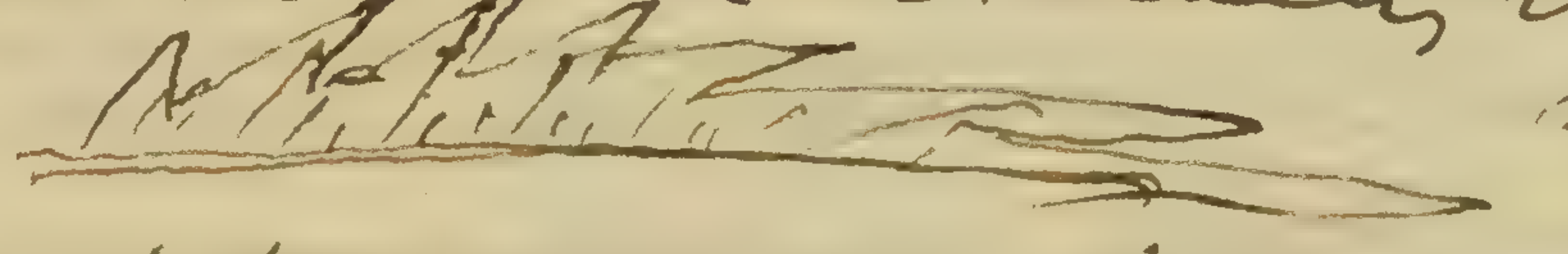
generally criminal, and
write to you about this, but as you pointed

mean when you say that the same laws of lobing that would apply to morus or vitis would not apply to the oak. That is primarily; but yet in a secondary sense, are they not the same? It seems just as in my laws of sex. I noted for a long time that the male flowers in ~~Copp~~ Carex and other things, were generally terminal, and wrote to you about this; but as you pointed out in reply it was not so in many sp. of Pinus — &c. It seemed as if there were varying laws; but really, as I saw at last, it was all one uniform law — the female was always in the lines of the most perfect nutrition. So in this matter of lobing: when the flow of nutrition is regular, and each vein gets its proper supply the outline is regular; but let some courses become obstructed, and they cannot develop, — lobing follows. The cut leaved Beech or Chestnut shows this very well, as the nerves are usually very regular.





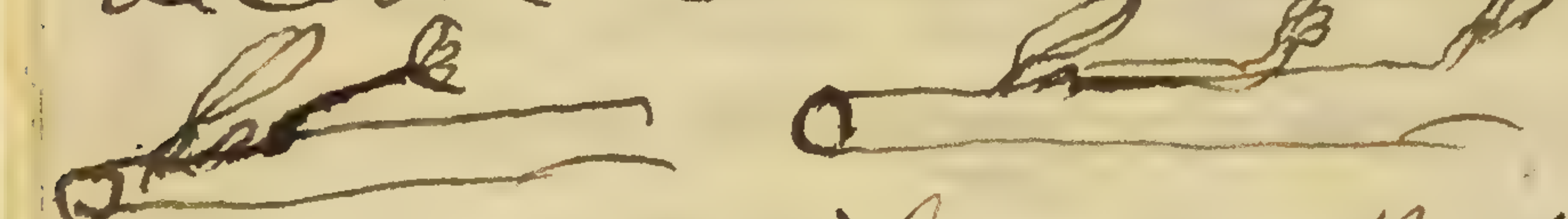
You see in the latter that the nerves have not developed and that thus the "cut" leaf follows. In the Gold Veined Honeysuckle, you can also see it very prettily illustrated. Will you say that this non-development of nerve is because the ~~leaf~~ blade matter is not developed? as Agassiz says or did say, of my law of sex, the male flowers (on monoecious plants) make

which covers it Dr Schlecht. yours E.
before the last the specimen from it was, but
I received the 1/4 earlier than are you

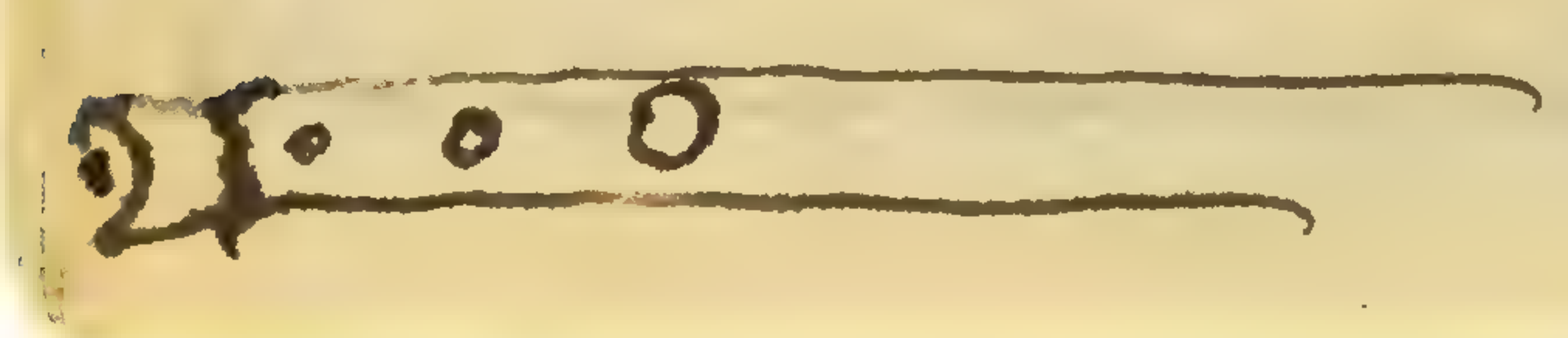
the weak branches, and not the weak branches make the male flowers! But, in the lobed leaf, I think it more reasonable to assume, if we must assume one way or the other, that the skeleton must be sketched out before the flesh which covers it.

I recieved the opuntia from Dr. Schott your E. Simpsoni, is now 1/4 larger than it was, but no signs of flower this season. are you sure that the O. vulgaris in Italy is not O. Rafinesqui also. I knew O. vulgaris in England as it was called there, and my memory of it as this O. Raf. O. vulgaris, as Dr. Schott sends me, so far as one may judge from a frond, I never saw. Yet of course I will not be certain after a lapse of memory over 25 years.

I know that the appearance of things ~~in an~~ in Solanaceae is an "old thing"; but I did ~~not~~ suppose my explanation of it was original. and if I understand you now you have not my idea of it. as I understand "Sendtners" "recaulescence" which I think is what you mean, a flower shoot from an axillary bud, has partially developed, but has become adnate with the main stem



as in the last "scratch". Now my idea is that it is simply a case of supra axillary buds, as we were looking at when you were: thus in Gymnocladus

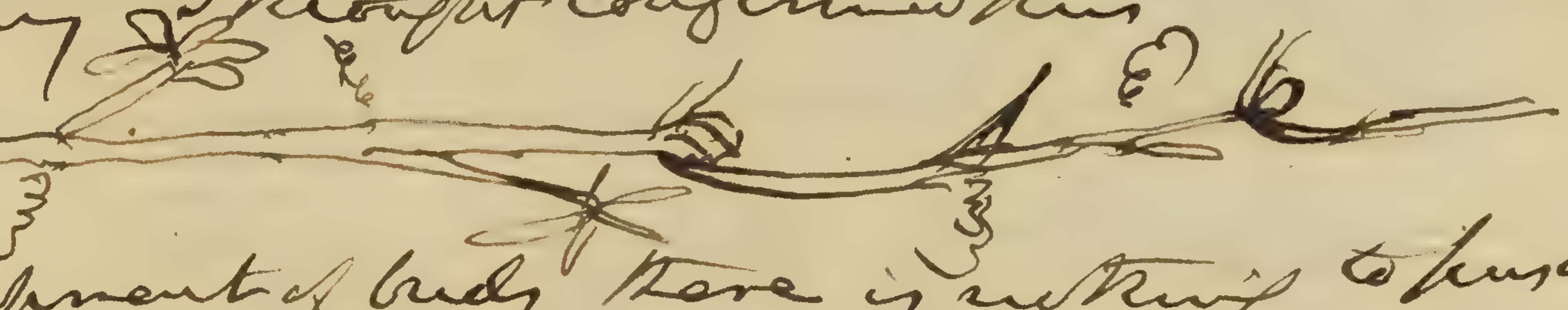


They are never constant

in relative distance. Sometimes the upper large ~~node~~ bud is almost opposite the next node above. If you look at the common eggplant, we see just the same thing; when young before much developed two axillary buds. Sometimes that upper (flowerbud) is almost close to the lower axillary bud, at other times almost up to the upper node. Where is the "adnation of the secondary to the primary axis" here? Where is the necessity for it, any such "adnating theory"? The whole seems simple and plain, and thus you see we need no special law for Solanaceæ, but we take the flower raceme in Phytolacca, and the tendril in the vine, all alike as developments of an upper bud in a set of supra axillaries. I have found in these supra axillaries that they are not organized as the lower ones are. They are generally the larger, and they produce the flowers. Though in Xylosteum the lower is the largest, yet it produces the flowers.

My idea was that not only in Ampelopsis, but in all plants, the leaf was terminal, — that the bud at its base was a new development, — and that it was the development of this bud, which forced the leaf out of the perpendicular direction. I could never account for the zigzag growth of most young shoots in any other way. In some cases —

Virgilia, Platanus, &c they remain terminal at the conclusion of the growth for the year. My Ampelopsis discovery I thought confirmed this.



no development of buds there is nothing to push out of line either succeeding node or leaf — the axial development alone (a bud of course originally) as pushed the leaf out a little, but gone straight on itself. Look at the young zigzag growth of Celtis! how is this accounted for unless the leaf is terminal?

Then your "laws of phyllotaxis" are not regular, even in the same ~~not~~ ~~individual~~ ~~to say nothing of~~, individual to say nothing of species. The very vigorous upright shoots rarely follow the same numerical arrangement as the weaker ones. This even in the Elms and Lindens which you refer me to. But perhaps the handiest illustrations of this are in Symphoria racemosa. In Paliurus and Zizyphus it is very striking; and Mr Ravenel tells me, but I have not myself seen, that in some plants even the growing spiral turn is sometimes to the right and sometimes to the left, in the same individual.

I am sure the fissures in bark are the results of the development of sclerous cells. I have now no doubt about this.

an account of going to Colorado. I fear I
shall not be at the meeting of Am. assn. at

MISSOU
ENGELMANN

On account of going to Colorado, I fear I shall not be at the meeting of Am. Assn. at Indianapolis; but I have sent them a paper " ~~On~~ the Monocotyledon, the primary type of all seeds"! Some time ago I sent specimens of my Cotyledons to Dr. Maxwell Masters, and he writes so singularly pleased with their novelty and interest, that I have been moved to generalize a little on them in this paper. It would seem that the division into Cotyledons is but an accident, and, in the majority of cases, of no more benefit to the plant than if the whole matter had remained in one mass.

I think I told you once I had found that the scales of Scotch Pine were double and inverted, and that the seed was only apparently on the upper side, but really on the lower, as in ferns the spores are; but if not I intended to do so. I see by a note in Silliman's journal recently Dr. Gray notices that this was discovered in Germany last year. The other part of his note — relating to Sciadopitys — I thought might have been more properly credited to Proc. Am. Ass. Chicago meeting.

My Abies leptolepis is having cones this year.

I mail a piece of the Opuntia from my garden, but I am sure it is some dried specimens rec'd a day before

originally from the same locality as that sent from Mr. Brick. among some dried specimens recd. to day from Lat. 40 in Colorado, I note a pretty Echinocactus — crimson — about two inches long — The asters you name I have growing from you. I will send you any living aster or Solidago I meet with. It is not much trouble to take up any I see. I have not been able to get the Pinus rigida for Ravenel — (in truth have been sick, several days in bed, and besides hard worked) but will do so next week. P. rigida here, opens its cones every year. I have noted that under culture, Pinus austriaca, is about ten years, in its juvenescent condition before it grows freely.

There may be some error about ~~Californian~~ cones of P. insignis, and others remaining ~~opening~~ closed for several years. P. pungens, P. inops and others open by drought, or frost (the same thing), and close again when the air gets moist.

But though I have much I would like to say, you will never get through so long a letter. ever truly yours
Thos Meehan

our party will go through St Louis and I hope they may stay long enough to let me see you - what is the number of your new house?

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Rec'd Sept 18
Ans. " 21

Germantown, 12 Sept 1871

Dear Dr Engelmann

I enclose you a rough draft of a paper I propose to offer to the academy nat. Science and should be obliged if you'd will first give me your opinion about it. I have examined the male catkins of C. avellana, C. Americana, and C. rostrata, but none have the ~~x~~ uniform scale. While I was in the Rocky mountains ~~those~~ the specimens I herewith send you, arrived. Hence they were not pressed. I add C. americana gathered here today.

Last week I was in Richmond Va, and was struck in riding on the R. R. with

the *aspect* of what I suppose
is *Pinus rigida*. I could
see no difference in their
appearance between them
and the P. *ponderosa's*
of the Rocky mountains.
The leaves were longer,
more flaccid, bluer green
and the cones (large) were
longer, and singly — never
in clusters. I had no
opportunity to handle
them.

Ever truly yours

Thomas Meehan

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Rec^d Dec 11th
Ans — 14

Germantown, Dec. 7th 1871

Dear Dr. Engelmann:

I have been at work the last three years getting the scattered collections in the Academy distributed through in their proper places in the General herbarium, and ~~adding the~~ adding to the index the genera new to the collection. I am anxious to be able to report to the Academy this month that this is completed, hence I have given every spare moment to the Academy determined not even to go over the naming of my Rocky Mt. plants till this is done. I have them placed in separate order sheets, this is all. I found only two Arceuthobiums, and I send you specimens of them with this. The short one is from Pinus ponderosa. The tree I gathered it

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from, seemed covered wholly
with berry bearing specimens.
Several days afterwards — per-
-haps 70 miles from the other
near the entrance to the South
Park I saw another which
I took to be ~~a new~~ another
species, on *Pinus contorta*. We
had no time to examine, only
to "grab" anything we saw
and run for the wagons, which
went continuously without
a halt, except at camp times;
but I cut off some — a piece
sent to day, and concluded it
was but a male form of the
other, until I saw *one branch
with a few berries* on, which I
also send. We were hurried as
I said — there might have been
more, but that was the only one
I could find in perhaps 3 minutes

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Germantown, ...187

search. Of the hazel one I have several pieces, but the one on the branch with fruit is that only one I referred to.

It is certainly very strange about the relationship of that Hagerstown Corylus to C. Avellana — I have written to my correspondent for further particulars about the extent to which it is distributed, and will let you know when I learn.

Ever truly yours

Thomas Meehan

a ball except at [illegible]
but I cut off some [illegible]
last a day [illegible]
[illegible]
[illegible] I [illegible] found
[illegible]
[illegible] were [illegible]
I said - there might have been
more but that was the only one
I could find in perhaps [illegible]

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Rec- Dec 21

Germantown, Dec 16 1871

Dear Dr. Engelmann

You are doing far more good with the arceuthobiums than I possibly could. So if you prefer to keep the specimen of A. am. on Pinus contorta do so by all means — you need send none of them back. A piece of the New York one would do just as well, and if you have two pieces so that I may present one to the academy's Herbarium so much the better, — or better still if after you have finished studying the genus you could send a whole set for the academy.

Very truly Yours
Thomas Meehan

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